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CAUSAL INFERENCE FROM CROSS-LAGGED CORRELATION COEFFICIENTS: FACT OR FANCY?

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Introduction

Several writers, some of them independently of each other, have suggested that the troublesome problem of inferring causal connections among variables from non-experimental survey data can be solved by interpreting patterns of so-called "cross-lagged" correlations among the variables, if the latter are observed at two or more points in time, (Campbell, 1963; Blalock, 1964; Pelz and Andrews, 1964; Farris, 1966; Yee and Gage, 1966; Rozelle and Campbell, 1966). Readers not familiar with the cross-lag method are referred to a description of it on p.10, below. None of the authors mentioned have, however, offered particularly convincing arguments why their cross-lagged method of inference should work. Appeal has simply been made to an inituitive "obviousness" of the assertion that any variable which causes another should be highly correlated with the second variable measured at some appropriately delayed point in time. According to which, by the logical fallacy of accepting the antecedent, the authors conclude that any two variables which are found to be correlated over time according to the specified pattern are *ipso facto* causally connected.

This paper is as a warning to those researchers who are tempted to use the cross-lagged method for causal inference without more adequately understanding its underlying assumptions and limitations. We shall undertake to show that, under some rather simple conditions, it is possible to "prove" just about anything with the cross-lagged method of inference.

The paper thus constitutes the point of departure for the author's proposed investigation of the analytical limitations of cross-lagged correlation analysis. But before we describe the new method it will be useful, as background for our discussion, briefly to consider the three methods that traditionally have been used for imputing causality to relationships among variables, on the basis of discrete observations of the variables' behavior.

Traditional methods of causal inference

EXPERIMENTAL MANIPULATION: The effect of one variable upon another may be inferred from the observation of non-chance differences in operational measures of the latter "dependent" variable, under different experimentally manipulated conditions (like "presence" versus "absence") of the former, "independent" variable -- provided that the effects of all other variables, including the mere passage of time, have either been "controlled" for (i.e., randomized or held constant) during the experiment, or can be assumed a priori to have no effect on the relationship among the variables under study.

The following table illustrates the different outcomes of an experiment that would lead observers to infer that necessary versus sufficient causal links existed between two variables, say A (here taken to be a dichotomous "independent", or manipulated, variable) and B (a "dependent", i.e. observed, variable which is also assumed to be dichotomous):

	B_o	Not-B_o
<u>If A_o</u> and all other variables that could possibly affect B have either been randomized or held constant	N_{11}	N_{12}
<u>If not-A_o</u> and all other variables that could possible affect B have either been randomized or held constant	N_{21}	N_{22}

<u>i</u>. <u>If $N_{11} = N_{12}$</u> (there is no "statistical difference" in the observed number of cases, N, in each row cell, i.e., the hypothesis that the observed difference could have arisen by chance cannot be rejected with a reasonable degree of confidence) <u>and if $N_{21} = N_{22}$</u>, then we conclude that A, together with all the other variables held constant during the experiment, have no (resultant) effect on B.

<u>ii</u>. <u>If $N_{11} \neq N_{12}$ and $N_{21} = N_{22}$</u>, then we conclude that A_o, perhaps in combination with any or all of the other variables held constant during the experiment, is a sufficient but not necessary condition for B_o or not-B_o, as the case may be, depending on whether $N_{11} > N_{12}$ or $N_{11} < N_{12}$)

<u>iii</u>. <u>If $N_{11} = N_{12}$ and $N_{21} \neq N_{22}$</u>, we conclude that A_o is a necessary but not sufficient condition for B_o (or not-B_o)

<u>iv</u>. <u>If $N_{11} \neq N_{12}$ and $N_{21} \neq N_{22}$</u>, we conclude that, for the particular values of the variables held constant during the experiment, A_o is a necessary and sufficient condition for B_o (i.e. A_o and only A_o causes B_o -- stochastically if N_{12} and N_{21} are significantly different fron zero).

Should the experimental variables <u>not</u> be dichotomous, one would merely add to the above matrix as many rows or columns as would be necessary to differentiate the n or m different values of A or B, respectively -- taking care always to include the catch-all control category not-A_i ($i = 1, 2, . . ,$ or n-1) to test the causal necessity hypothesis.

Notice that experimental manipulation of A will yield no information about whether or not B also causes A. In other words, if B in fact did cause A in "the real world", an experimental intervention using A as its independent variable would provide not the slightest clue about such a feedback effect of B on A. Thus generalizing from the results of a one-way experiment to the world at large is precarious business if feedback among the variables studied is likely to occur.

Why not therefore, one could argue, simply run <u>two</u> experiments for each pair of variables studied, each experiment set checking on each suspected causal direction? Unfortunately, the cumulative effect of non-linear feedback among two or more variables will generally be different from any simple algebraic "sum" of the experimentally, separately determined one-way effects of one variable upon the other. "Causation" between A and B, say, in a non-linear feedback loop is likely to vary, seeming tc go at times in one direction, then in another, depending on the particular dynamic state of the A-B system at any specific point in time. This phenomenom will be demonstrated below (p.13).

The chief characteristic to be noted from our brief overview of the experimental method of causal inference is that it requires one to make the a priori assumption that the causal link he is about to study experimentally is in fact uni-directional. Feedback among the variables, such as is commonly found in dynamic systems, simply needs to be assumed away before valid causal inferences can be made from observed differences in the "effects" of "independent" variables on "dependent" ones.

Let us in closing consider the merits of an often heard critism, that traditional experimental methods are "static", i.e. cannot be used for unravelling dynamic, i.e. time-dependent, relationships among variables -- always given the assumption that these dynamic relationships are in fact unidirectional. This, however, is a spurious criticism of experimental method. There's nothing preventing an experimenter from adding time-subscripts to his "dependent" observations (in our example, above, we might have tried relating A_o to $B_{o,t+2}$). Yet traditionally it appears that most experimenters have simply assumed that the effect of A on B, if there was one, would have worked itself out sufficiently quickly to have stabilized, or become significant, in the time delay needed to perform the experimental measurements.

MODEL CONSEQUENCE REJECTION: In cases where direct experimental manipulation is impossible, or for some reason prohibitive, resort is often taken to the experimental manipulation of symbolic models of the real situation. Thus, given a symbolic model of the presumed relationships among two or more variables, mathematical analysis may yield one or more non-obvious, potentially surprising, consequences of the postulated model. If empirical observations of the relevant variables then turn out to be "sufficiently similar"

in pattern to the predicted relationship, the analyst will not reject his model, i.e. will keep on believing in the casual connections between the variables that he has described therein. If, however, empirical observation does permit him to reject his derived hypothesis -- and included with the latter the primitive assumptions and operational definitions that building the model had forced him to make -- the analyst must now revise his model in order better to fit the data. At this point he must go through the predictive consequence-testing cycle all over again, until at some point he has satisfied himself, and his critics, that he has checked out his model in all its many possible (in practice only the more sensitive) state variations.

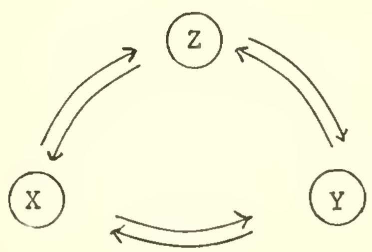
We shall not bore the reader with lengthy illustrations of causal inference through model building. Simon in Models of Man provides easy-to-follow demonstrations of specific applications of the method. Yet it may be useful to note in passing that a simple way of obtaining "potentially surprising consequences" from a model is to employ two different models -- both of which should appear a priori to be just as reasonable to persons familiar with the subject matter under study -- which just happen to yield contradictory predictions about the behavior of certain variables under some observable set of circumstances. Then, whatever the outcome of observations, one it bound to obtain a surprising, i.e. "significant", result.

STATISTICAL EXPERIMENTATION: If it appears a priori reasonable to view the interactions among a small set of variables (say three of them) as constituting a closed system, i.e., as being largely independent of variations in any other ("exogneous") variable over the period of observation, then, if all (three) variables are significantly correlated, it is in some circumstances

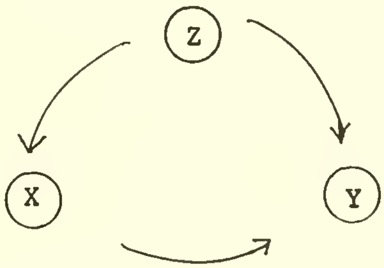
possible, through _post hoc_ partical correlation analysis, by making some additional _a priori_ assumptions about the time precedence among subsets (pairs) of the variables, to rule out some of the possible causal explanations that would be compatible with the observed pattern of co-variation, and perhaps thus, by elimination of all but one, establish _the_ causal relationship that must exist in the system, (Simon, 1957).

Illustration of this method of _post hoc_ statistical experimentation will be useful for our later discussion. Consider the variable system x, y, z; which, as noted, we may view as a closed causal system by assuming that the error terms in the variables are uncorrelated -- an assumption we could check empirically if we were willing _a priori_ to specify the causal shape of the system. Existence of correlated error terms in two or more variables would indicate the presence in the system of one or more unrecognized variables, which thus were exacting a systematic effect on the hitherto included variables. Such unrecognized variables would then have to be identified (not always easy) and included in the "closed" system -- thereby usually expanding by orders of magnitude the number of possible causal hypotheses in the system -- in order to make our causal inferences from partial correlation analysis valid.

Given the above closed system assumption, the following diagram generates the 27 different causal explanations -- ways in which each variable could be linked to the other. all compatible with an observation of three significantly non-zero first-order correlations among three variables:



In order to establish a unique causal explanation by means of partial correlation analysis, it turns out, like for the case of experimental manipulation, that the existance of feedback loops among the variables must be ruled out a priori by making a suitable set of assumptions. Say for illustration we assume that X and Z both precede Y in time, such that, since time is uni-directional, Y cannot possibly "cause" either X or Z. Similarly we may be able to state that Z always precedes X. The remaining causal connections in our completely intercorrelated three-variable system would then reduce dramatically to:



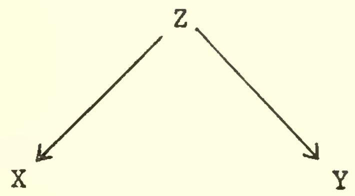
First-order partial analysis may now be able to establish which of the three remaining causal link patterns, implicit in our last graph of the system, could possible have produced the three observed zero-order correlations. The definition of the first-order partial correlation of X versus Y, "controlling for" Z, can be written:

$$r_{XY\cdot Z} = (r_{XY} - r_{XZ} \cdot r_{YZ})/(1 - r_{XZ}^{2})^{\frac{1}{2}}(1 - r_{YZ}^{2})^{\frac{1}{2}}$$

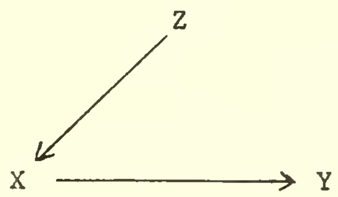
Thus one observes that whenever the first-order correlation coefficient between two variables, controlling for the effect of a third variable on both of them, goes to zero, then this result may be taken to indicate that the observed zero-order correlation had been spurious, i.e. had been caused by the simultaneous operation on the two faced variables of the third, now "controlled" variable.

Hence, to illustrate, given the remaining three causal explanations of complete intercorrelatedness that are embodied by our last graph, we obtain the following three decision rules:

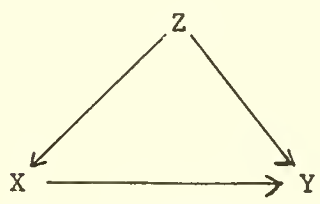
<u>Case 1</u> $r_{XY \cdot Z} = 0$ implies:



<u>Case 2</u> $r_{XY \cdot Z} \neq 0$ but $r_{ZY \cdot X} = 0$ implies:



Case 3 $r_{XY \cdot Z} \neq 0$ and $r_{ZY \cdot X} \neq 0$ implies:



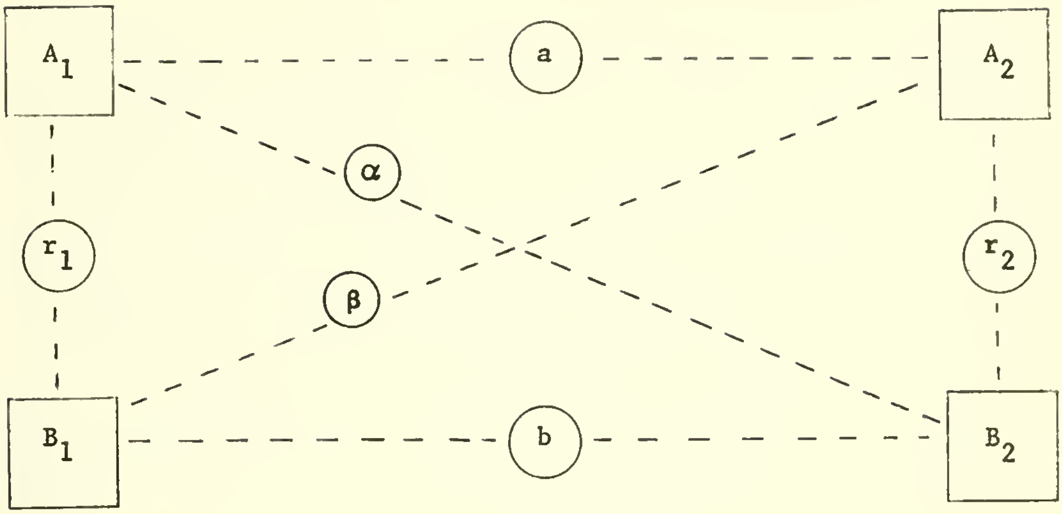
Any other combination of causal links and first-order partial correlations would be incompatible with <u>i</u>. the initial observation of significant zero-order correlations among all three pairs of X, Y, and Z; or <u>ii</u>. our <u>a priori</u> assumptions about the causal time-precedence, <u>i.e. absence of feedback</u>, between each variable pair.

However, we ought also to note at this point that the use of correlation coefficients of any sort forces one to assume that the underlying relationship between the correlated variables is linear (or else that the variables have been suitably transformed to correct for non-linearity), and that the time period between the correlated observations in each of the variables (usually taken to be instantaneous, such that all matched observations are made simultaneously) reflects the "true" time period in which the influence of one variable on the other will largely have been affected. Violation of either of the latter sets of assumption would further invalidate causal inference by statistical experimentation through partial correlation analysis.

Cross-lagged correlation analysis

The cross-lagged correlation technique presumably tests for time precedence among variables (which, as we saw, had to be assumed *a priori* for partial correlation analysis to work) while *simultaneously* measuring their strengths of association. For the single two-by-two case the cross-lagged method of causal inference can be described as follows (paraphrased from Pelz and Andrews, 1964):

Assume the existence of a system of two variables A and B -- i.e. no other variables impinge on A and B. In other words, as stated above, the error terms of A and B, given the hypothesis about their causal connection, are assumed to be uncorrelated. Measure the variables at two points in time: A_1, A_2, and B_1, B_2. One can then obtain the following set of correlation coefficients, (note that the dotted lines in the figure below no longer represent causal links among the variables):



The proposed method of cross-lagged correlation analysis now asserts: If A and B are causally related then the simultaneous zero-order correlation coefficients, r_1 and r_2, "will both be positive and about the same magnitude" (Pelz and Andrews, p. 837). But this assertion is false, as will be demonstrated below.

Moreover, "the horizontal (or lagged) correlations . . . . will reflect the consistency in each variable over time" (Pelz and Andrews, ibid.) In time-series analysis the horizontal coefficients a and b are usually labeled "auto-correlation", which to be sure does reflect a "consistency" of sorts among the variables. But auto-correlation is not such a desirable property for analysts to have as Pelz and Andrews seem to imply. Usually auto-correlation in time-series indicates trouble for most statistical techniques, which, like correlation coefficients, which depend for their validity on independently drawn samples of observations. Pelz and Andrews, however, indicate no need for adjustment of one's conclusions from cross-lagged analysis due to the existence of autocorrelation in the data.

The heart of the proposed method of causal inference from patterns of cross-lagged correlation coefficients is found in the following interpretation given by the authors to the remaining two of the six correlation coefficients displayed in Figure 1, namely α and β. (The decision rule below is paraphrased from Farris, op. cit. p. 142):

Case 1a: If $\alpha \neq 0$ and $\beta = 0$, then A causes B.

Case 1b: If $\alpha = 0$ and $\beta \neq 0$, then B causes A.

Case 2a: If $\alpha \neq 0$, $\beta \neq 0$, and $\alpha > \beta$, then A causes B more than B causes A.

Case 2b: If $\alpha \neq 0$, $B \neq 0$, and $\alpha < \beta$, then B causes A more than A causes B.

Case 2c: If $\alpha \neq 0$, $\beta \neq 0$, and $\alpha = \beta$, then A causes B as much as B causes A.

Farris does not mention the sixth possibility, namely, that both $\alpha = 0$ and $\beta = 0$. Perhaps this case were not thought possible, given the observation of a significant r_1. (We shall demonstrate below that indeed it is possible.) However, simple extrapolation of the above "logic" of cross-lagged analysis suggests that our inference for this sixth case should be:

Case 3: If $\alpha = 0$, and $\beta = 0$, then A and B are causally unconnected.

This, perhaps severly paraphrased, is as much analysis as the authors offer to support their contention that cross-lagged correlation patterns will be valid indicators of the underlying causal connections among variables that are observed at two or more points in time.

A simulation experiment

To set the stage for our proposed investigation of conditions that define valid uses of the cross-lagged method of inference, we carried out the following simple experiment:

Consider an artificial computer system consisting of only two variables, A and B (we call them X and Y). To make things just a little bit interesting we prescribe the following simple-minded relationship among X and Y:

$$Y_t = k_1 X_{t-6} + \epsilon_1$$

$$X_t = k_2 Y_{t-3} + \epsilon_2$$

where k_1 and k_2 are constants, and ϵ_1 and ϵ_2 are uniform random deviates with mean zero and fixed ranges. In other words, our little simulated world will exhibit simple harmonic nine-period periodicity. To keep things clean we applied cross-lagged analysis to data generated by only one such harmonic period. Let us label our terms:

<u>X</u>: average index of satisfaction, running from 0.00 to 2.00 (Lo, Medium, Hi), of individual research scientists working in electronic widget companies.

<u>Y</u>: profits generated (in $-thousands) by the new widget products invented and patented by the firm's research labs each year. (This is our measure of scientific productivity.)

<u>Period analyzed</u>: 1956 to 1965 inclusive (i.e. X_1 to X_9 and Y_1 to Y_9).

<u>Sample size</u>: 30 widget companies that each employ (by convenient government regulation) exactly 5 research scientists (whose individual satisfaction scores were averaged each year).

All we now need is a set of initial values for the X's (or the Y's), in order to start our simulated world off and running. We chose to generate a time-series by the following set of equations:

$$X_{1,2,3,7} = \epsilon$$

$$X_{4,6,8} = a(X_{t-2} + X_{t-3}) + (1-a)\epsilon_i$$

$$X_{5,9} = b(X_{t-4} + X_{t-8}) + (1-b)\epsilon_i$$

The random numbers ϵ_i were drawn from a Rand Table of Uniformly Distributed Deviates in the Sloan School Computer Facility.

Our sample data average satisfaction (X) and productivity measurers (Y) for the thirth electronic widget manufacturers over a nine year period,"1956 to 1964, are presented below.

1956	1957	1958	1959	1960	1961	1962	1963	1964
X1	X2	X3	X4	X5	X6	X7	X8	X9
.8030	.4797	.7344	.8817	.8783	1.1635	.5870	1.0693	1.3280
.4410	.0624	.3104	.5178	.2848	.5900	.5430	.5321	.6707
.1250	.6591	.5984	.4625	.1112	.8214	.4010	.5593	.1819
.6360	.0832	1.4912	.6121	.5639	1.9210	.8440	1.2976	1.0162
.6110	1.2597	.0704	1.1355	.5263	.5185	.7450	.5932	.8881
.1540	.9672	.8416	.7637	.2178	1.3148	.5350	1.0403	.3929
.9450	.7436	1.3712	.9734	.9186	1.9341	.8750	1.6035	1.3808
.4240	1.0543	.9936	.8719	.3800	1.4057	.2110	1.1069	.7145
.2350	.3081	1.3296	.4833	.2201	1.7875	.3760	1.2761	.4222
.0440	1.2155	.3984	.7784	.1551	.7493	.2440	.6226	.1835
.0050	.6487	1.2240	.4605	.0303	1.6592	.5990	1.0374	.1794
.3590	.8203	.8480	.8019	.6554	1.2212	.5430	1.0629	.7520
.5980	.1040	1.3424	.4027	.5039	1.6752	.4280	1.2267	.9452
.4600	.7644	.8368	.7929	.4215	1.3384	.8670	1.1044	.6577
.3210	1.0270	.0944	.9055	.3529	.4358	.4100	.5547	.5983
.6920	.4498	.2240	.7549	.3757	.5647	.0410	.6437	.8743
.1950	.8502	.2288	.5437	.5046	.5507	.7940	.5594	.6511
.4510	.7163	.7888	.8199	.4647	1.1573	.4640	.9386	.7863
.9480	.8359	.3184	1.0409	.5190	.7070	.5130	.7644	1.1972
.9800	1.1206	.7712	1.2600	.5730	1.3123	.0440	1.1101	1.2634
.3310	1.1063	.9968	.8100	.6665	1.4570	.4980	1.3494	.8058
.8090	.6630	.0640	.9464	.5741	.4876	.7420	.5560	1.0155
.7970	.6812	1.4624	.7871	.8872	1.9348	.9660	1.7238	1.2289
.1860	.4329	.4048	.5293	.1634	.6331	.7130	.4404	.2554
.7400	.3822	.6240	.6555	.6393	.9601	.7090	1.0342	.9958
.5410	1.0192	.8976	.8581	.7896	1.3392	.4300	1.2455	1.0399
.1160	.8372	1.5808	.5871	.0956	2.0940	.2750	1.3825	.1797
.4830	.3211	1.2672	.6341	.3543	1.7257	.1000	1.1411	.6886
.6900	.9737	.6608	1.0957	.6608	1.2377	.7630	1.1323	1.0372
.0910	1.1466	.0144	.8338	.4947	.3461	.2700	.5469	.4354

1956	1957	1958	1959	1960	1961	1962	1963	1964
Y1	Y2	Y3	Y4	Y5	Y6	Y7	Y8	Y9
43.9839	45.5353	68.5929	30.6735	52.0452	54.7294	29.5800	19.9408	33.3080
26.8987	14.8456	36.5103	27.9180	27.0053	27.4753	19.3480	6.7122	17.7380
21.6015	6.9588	50.2256	21.6755	27.4610	8.9480	4.6800	29.1910	29.1780
29.9612	29.2960	9.4317	42.0310	61.6101	42.3745	22.9840	4.7331	68.2590
53.3133	27.9823	28.2529	41.9650	27.3905	36.6625	23.5240	54.2668	7.9080
35.0625	11.5731	74.4924	28.5010	49.8895	17.9495	7.1200	42.2901	38.8720
45.4569	49.9092	5.8536	45.0945	73.4895	59.3931	36.8120	32.7663	64.0440
43.2667	19.1950	78.6656	13.1560	51.9132	31.0051	18.8880	43.9411	45.1570
23.2174	13.2294	98.9499	20.2840	58.5375	17.8623	11.3920	16.5370	60.8220
39.2643	11.1314	43.7069	16.8190	32.8912	7.7787	3.9480	52.6477	22.5930
22.1252	4.7503	91.3138	29.8375	48.1084	8.9840	2.9200	28.3378	55.9050
38.9945	37.2902	68.4990	31.1080	52.3499	34.5961	14.4000	35.2381	38.5750
22.7928	26.3480	92.1372	22.0990	57.9301	42.4178	24.4760	7.0785	63.1480
39.1373	20.9687	77.4018	45.3035	52.6561	28.0101	19.2840	32.3397	39.4410
41.1760	19.3770	24.3150	24.6675	27.8815	28.6377	13.9640	45.0225	7.5680
35.8769	21.9876	32.4960	5.9565	32.5848	38.2923	27.8400	19.8999	14.4250
29.2838	25.2276	33.1479	41.1785	25.2058	30.1543	8.5880	35.0226	10.8860
41.5666	24.7871	62.7611	27.1260	43.6794	32.9778	17.9240	31.2466	36.4060
50.2241	31.1850	38.4422	26.1635	39.2580	49.1796	37.2760	35.9644	15.2830
58.4803	30.0465	76.5329	2.6785	52.4413	52.0963	35.3120	47.2113	37.8490
40.1771	35.3017	79.8279	27.9565	63.7797	32.8496	12.8440	48.0451	47.8560
43.1779	30.5874	32.0929	39.3910	29.7515	43.0511	30.0080	27.8730	6.2900
39.8651	47.2384	7.3759	52.5250	77.8430	50.5057	28.8560	30.9366	69.4580
23.9738	12.0672	37.0523	38.9675	23.7872	10.6154	8.4680	20.0074	22.1860
31.1702	33.8918	52.5697	39.9905	49.9333	42.5100	29.8600	17.5131	31.1950
39.9746	43.3092	77.8703	22.7095	57.0965	43.4185	20.0680	44.1441	40.6570
31.3227	8.3371	16.9560	17.6330	64.8941	10.5242	5.7920	37.4481	72.2760
31.3383	21.3163	93.8971	7.5460	52.3318	28.4289	19.6320	15.9700	61.9590
51.8782	37.1230	72.6357	41.6295	54.5413	43.3497	26.1280	43.7728	30.4160
41.3288	27.2651	22.4190	13.3760	26.0286	19.2247	3.4200	50.7483	1.4330

The correlation between Satisfaction (1956) and Productivity (1956) turns out to be .58. We will now compute different sets of cross-lagged correlations, and use the above described Farris' algorithm to determine whether Satisfaction causes Productivity, or vice-versa, or what not. Let us first examine the cross-lagged correlations over a three-year lag, fro 1956 to 1959:

Table 1.	Satisfaction 1959	.02	Productivity 1959
Satisfaction 1956	.58		.21
.58			
Productivity 1956	.99		-.16

Obviously, according to the cross-lagged algroithm, Productivity causes Satisfaction (.99 versus .21). Nevertheless, just to corroborate this result, let us examine the same correlations for a six-year lag, say for the period 1956 to 1962:

Table 2.	Satisfaction 1962	.20	Productivity 1962
Satisfaction 1956	.21		.99
.58			
Productivity 1956	.01		.55

The conclusion from this table, alas, would have to be the reverse of the first table: Satisfaction according to Table 2 obviously causes Productivity, (.99 versus .01). This is disturbing. Let us, therefore, look at the following years figures, i.e. 1956 versus 1963:

Table 3.	Satisfaction 1963	-.10	Productivity 1963
Satisfaction 1956	.31		-.17
.58			
Productivity 1956	.07		.63

However, this time we must conclude, using the cross-lagged argument, that there is obviously no causal connection between Satisfaction and Productivity. Looking at this table alone, one might speculate that the high correlation (.58) between Satisfaction and Productivity was spuriously due to the effect of some third variable on both. (But this we know to be nonsense, since the only two variables in our simulated system were X and Y.)

Well, let's be conservative then; let us only take one small step at a time, What about the 1956 versus the 1957 figures?

Table 4.	Satisfaction 1957	.09	Productivity 1957
Satisfaction 1956	-.15		.74
.58			
Productivity 1956	.65		.56

Aha. Here's a big effect both ways (.74 and .65). Obviously, Productivity causes Satisfaction as much as Satisfaction causes Productivity, which seems only natural from what we otherwise know about these variables. So now let us look at the 1956 versus 1958 correlations:

Table 5.	Satisfaction 1958	.98	Productivity 1958
Satisfaction 1956	.03		.10
.58			
Productivity 1956	-.28		-.16

Oops. No effect again, (.10 and -.28). But.... Satisfaction did relate quite well to Productivity (.98) in 1958! "That's at least something." Only too bad the 1957 intra-year correlation was so low, (.09).

Well, the reader may object, we are rejecting a promising method of inference on the basis of a single, perhaps pathological sample of observations. So we let our equations generate six random samples of 30 electronic widget manufacturers, whose research scientists behave according to our simple psycho-economic "law", (next page).

Table 6.

Sample 1	Y1956	Y1957	Y1958	Y1959	Y1960	Y1961	Y1962	Y1963	Y1964
X1956	.58	.74	.10	.21	.32	.94	.99	-.17	.05
Y1956	.58	.65	-.28	.99	.55	-.16	.01	.07	.60
	X1956	X1957	X1958	X1959	X1960	X1961	X1962	X1963	X1964

Sample 2	Y1956	Y1957	Y1958	Y1959	Y1960	Y1961	Y1962	Y1963	Y1964
X1956	.71	.70	-.07	-.10	.22	.93	.99	.35	-.21
Y1956	.71	.85	-.13	.99	.50	.06	-.06	.27	.70
	X1956	X1957	X1958	X1959	X1960	X1961	X1962	X1963	X1964

Sample 3	Y1956	Y1957	Y1958	Y1959	Y1960	Y1961	Y1962	Y1963	Y1964
X1956	.60	.67	.03	.01	.28	.91	.99	.03	-.05
Y1956	.60	.76	-.18	.99	.41	-.05	-.12	.14	.56
	X1956	X1957	X1958	X1959	X1960	X1961	X1962	X1963	X1964

Sample 4	Y1956	Y1957	Y1958	Y1959	Y1960	Y1961	Y1962	Y1963	Y1964
X1956	.75	.49	.04	-.22	.17	.92	.99	.36	-.15
Y1956	.75	.83	-.06	.99	.65	.17	-.17	.39	.80
	X1956	X1957	X1958	X1959	X1960	X1961	X1962	X1963	X1964

Sample 5	Y1956	Y1957	Y1958	Y1959	Y1960	Y1961	Y1962	Y1963	Y1964
X1956	.77	.74	.11	-.23	.37	.95	.99	.32	-.59
Y1956	.77	.78	-.04	.99	.61	.12	-.19	.31	.73
	X1956	X1957	X1958	X1959	X1960	X1961	X1962	X1963	X1964

Sample 6	Y1956	Y1957	Y1958	Y1959	Y1960	Y1961	Y1962	Y1963	Y1964
X1956	.68	.75	.11	-.20	.41	.94	.99	.26	-.01
Y1956	.68	.84	.21	.99	.60	.39	-.19	.55	.69
	X1956	X1957	X1958	X1959	X1960	X1961	X1962	X1963	X1964

Examination of Table 6 reveals two clear characteristics of the computed cross-lagged correlation coefficients. (To check one's comprehension of Table 6, the cross-lagged correlation "X1956 versus Y1960" is .32 in the first sample, and .28 in the third sample, whereas the cross-correlations "Y1956 versus 1960" are .55, and .41, respectively.) The first characteristic we note is that the causal inferences drawn by the proposed cross-lagged heuristic indeed do vary all over the map, depending on the time periods chosen, for all six samples. The arrows in the margins display the causal directions that would be indicated by cross-lag analysis, (.55 was arbitrarily chosen as a significant correlation coefficient cut-off point).

The second characteristic that stands out of Table 6 is that the pattern of different "causal directions" suggested by each of the nine time-period sets is quite stable from sample to sample. This is hardly surprising to anyone familiar with the laws of probability sampling. We should obviously have been disturbed had the time path of our little dynamic world -- as this gets described by the cross-lagged correlation coefficients -- not been fairly comparable for each random sample of 30 input sets drawn.

Much more interesting is the question, given this stable pattern of cross-lagged correlation coefficients over the nine year period, would the reader be able to infer from this the underlying causal structure of our "psycho-economic law" that generated the data? The author's answer is, from cross-lag coefficients alone, hardly.

The next most interesting question is, to what extent is the psycho-economic law described by our X-Y equations above pathological, far-out, and unrepresentative of cases found in the real world of behavior? Our answer,

presented here without proof, is that the lability of our conclusions drawn by the cross-lagged method of causal derives largely from the non-linear feedback that is taking place between the variables.

Our assertion is two-fold: First that non-linear feedback among the variables is a common social science phenomenon. Secondly, that most non-linear feedback systems have the nasty characteristic of appearing, to anyone who merely "snap-shoots" its behavior by selected point-in-time observations, as if it were some sort of schizoid, flip-flop mechanism: At times one variable appears to be driving the system, at times it's another variable in the driver's seat, and at still other times the system's variables appear to have no systematic relationship to one-another. In other words, the meaning of "causal links", in the traditional uni-directional sense of the word, has simply disappeared. We are left with having to describe whatever non-linear behavior we wish to understand in terms of similarly non-linear simulation models, (Forrester, 1961).

What the latter assertion implies for the worth of the traditional two-by-two correlational "findings" reported by most survey research studies, in which feedback among the variables may be occurring, should be fairly obvious.

Conclusion

We noticed, what we had not realized so clearly before, that the classical approaches to causal scientific inference, namely experimental method and partial correlation analysis (i.e., statistical experiments), both assume a priori that there exists no feedback among the variables studies, i.e., that "dependent" and "independent" variables can be uniquely differentiated. To the extent

that feedback <u>does</u> exist among the variables, particularly feedback of the non-linear variety -- in which relationships among variables are not monotonic -- inferences from laboratory experimentation or statistical correlation analysis stand in danger of being highly misleading.

Physically based sciences have escaped the more devastating errors introduced by non-linear feedback inferences from experiments: In the physical world some types of variables just cannot affect other variables in any one chain of reactions. Social scientists are liable to find themselves with no such luck. Most social behavior, like for instance organizational behavior, is highly interactive, and all too frequently seems better described as a non-linear feedback system. The author has therefore proposed to investigate (with George Farris) the sensitivity of the cross-lagged correlation method of causal inference to different classes of non-linear feedback, in order to try to identify, by means of suitable *a priori* tests and system diagonstics, the possibly valid areas of application of the cross-lag method in behavioral science.

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